peculiar structure project. These long hairs are very thin at the bulb, and increase very gradually in thickness for about one-third of their length, when they suddenly contract a little, and then expand into a flat lance-shaped blade, which terminates in a very fine point. This coarser part covers the whole body, the thick root of the tail, and the upper part of the limbs; the rest of the tail, the under side of the muzzle, and the upper surface of the feet are clothed with short, close hairs. The ears are of moderate size, the eyes very small, and the toes on all the feet, five in number, are armed with small sharp claws, and without webs, but the second and third toes on the hind feet are united as far as the end of the first phalanx.

The most remarkable peculiarity of the animal is its tail, which presents a most unusual development for an insectivorous mammal. Prof. Allman says:—"It is so thick at its base that the trunk seems uninterruptedly continued into it; but it soon becomes laterally compressed, and then grows gradually thinner and narrower towards the tip. . . . . Its lower edge is rounded, and its upper is continued into a membranous crest about one-eighth of an inch in height, and clothed with the same short, stiff, appressed hairs" as the rest of the tail.

This great development of the tail might of itself convince us that this organ is of great service to its owner, and such, from the account of the habits of the animal

FIG. 3.—West African River Shrew (*Potamogale velox*).

given by its discoverer, is evidently the case. M. du Chaillu says:—"This extraordinary animal (Fig. 3) is found in the mountains of the interior, or in the hilly country explored by me north and south of the equator. It is found along the water-courses of limpid and clear streams, where fish are abundant. It hides under rocks along these streams, lying in wait for fish. It swims through the water with a rapidity which astonished me; before the fish has time to move it is caught. On account of the rapidity of its movements I have given it the specific name of *Velox*. The animal returns to land with its prey almost as rapidly as it started from its place of concealment. The great motive power of the animal in the water seems to be in its tail."

So far as we have been able to read over this volume, we have found that great pains have been taken to record all the novel facts known about the animals here treated of. We perceive an account of the nest-building power of that most extraordinary Madagascar lemur, the Aye-Aye (*Cheiromys madagascarensis*) and the strange instances of mimicry about the bats, first noticed by Dr. Dobson, is to be found also noticed.

An index to each volume would be a very desirable addition.

NITRIFICATION

THE origin of salpetre is a subject which has vexed the minds of several generations of chemists. Nitrate of potassium, or salpetre, is found in nature as a white crust, appearing on certain rocks, old walls, and even upon the surface of the soil; from this mode of occurrence the name "salpetre" is doubtless derived. The largest natural source of salpetre is afforded by certain soils in India. Soil having a white film of salt on the surface is collected from the neighbourhood of house-drains and stables; the soil is washed with water, and the nitre crystallised from the solution. With this Indian salpetre England has been, till quite recently, almost exclusively supplied. The countries of Continental Europe, not having access to so considerable a natural source of nitre, have been obliged from early times to produce nitre for themselves. At first the earthen floors of cottages and stables were collected, washed, and nitrate of potassium obtained by treatment with wood-ashes and crystallisation; but the inconvenience of collecting such material, and its general poverty in nitre, soon led to attempts

at producing salpetre by artificial means. To Glauber, a chemist of the seventeenth century, apparently belongs the credit of first preparing nitre artificially. The process as carried out in the present day is in outline as follows:—Soil, containing more or less of vegetable mould and carbonate of calcium, is mixed with a certain proportion of stable manure or other refuse animal matter, and disposed in small heaps, care being taken that the mass of soil and manure shall be sufficiently porous to ensure the free admission of air: these heaps are protected from rain, and are from time to time watered with stable sewage. At the end of two or three years the earth is sufficiently rich in nitre to be worth extracting. This tedious process for manufacturing nitre has, during the last few years, been superseded to a considerable extent by the treatment of Peruvian nitrate of sodium with chloride of potassium, by which nitrate of potassium and chloride of sodium are produced.

It is evident that the artificial nitre-beds just described, merely perform, on an exaggerated scale, an operation which occurs naturally in all ordinary soils. The chemical analysis of drainage waters has taught us that such waters are characteristically rich in nitrates, and that the amount of nitric acid present stands generally in close relation to the quantity of nitrogenous manure previously applied to the soil. The published analyses of the drainage waters from the experimental wheat-field at Rothamsted, show that ammonium salts applied as manure are rapidly converted into nitrates by the soil, the quantity of nitric acid in the drainage water being proportional to the amount of ammonium salt applied. The recent application of soil for the purification of sewage is another striking example of the same action. The sewage, as poured upon the soil, contains ammonia, and putrescible organic matter rich in nitrogen; the sewage which has filtered through a few feet of porous soil is found to contain nitrates, but only traces of organic nitrogen or ammonia.

What explanation can we give of this phenomenon of nitrification? It is clearly a process in which nitrogen is oxidised into nitric acid; but how is this oxidation brought about? The old chemists believed that a decaying organic body evolved more or less of its nitrogen in a free state, and that this nitrogen, while nascent, combined with the oxygen of the air to form nitric acid. This view has been held by some down to the present day. Hofmann, in his Exhibition Report of 1862, offers the same explanation, only substituting for free air the oxygen condensed on the surface of porous bodies. This theory has been extended by some to include the ordinary nitrogen of the atmosphere, so that on their view nitric acid may be formed in soil from the nitrogen and oxygen of the atmosphere, without the intervention of other nitrogenous matter. According to others the oxidation of gaseous nitrogen is brought about not by ordinary oxygen, but by ozone. Other chemists have inclined to the belief that nitrogen is never oxidised in the soil except when in the form of ammonia, and that the nitrogen of organic matter is always converted into ammonia as a preliminary to nitrification. According to some experiments, the ferric oxide, which gives a red colour to so many of our soils, is itself an oxidising agent, and capable of converting ammonia into nitric acid.

We need not, however, enumerate all the opinions that have been held on this confessedly obscure subject. Many of the experiments which were thought to support certain views, now appear, in the light of recent evidence, of little value. Before, however, discussing the new facts recently contributed to the subject, we may just indicate those points which have been most clearly established.

There is very little evidence for supposing that gaseous nitrogen is ever converted into nitric acid in the soil. Nitrous and nitric acid are indeed produced by electric discharges through the atmosphere, thus originating the small amount of nitrates brought to the soil by rain, but this appears to be the only reaction capable of producing nitric acid from the direct union of oxygen and nitrogen. According to Carius even ozone is quite incapable of oxidising gaseous nitrogen. Ammonia is, on the other hand, oxidised by ozone, nitric acid being formed; but that ozone is an agent in soil transformations is certainly unproved, and appears very improbable. There remains the action of ferric oxide, already referred to. This reaction deserves further study; it cannot, however, be considered as generally important, since nitrification certainly occurs with vigour in soils practically destitute of ferric oxide.

The researches of successive generations of chemists had thus failed to give any satisfactory explanation of the important phenomenon of nitrification. The subject has quite lately been attacked by Schloesing and Müntz from an entirely new point of view; their results, published in the early part of last year, plainly indicate that nitrification, instead of being brought about by purely chemical forces is, in fact, the work of a living organism. The evidence adduced in support of this new view is very simple. These chemists show that nitrification, however active, is immediately stopped by the vapour of chloroform, a substance which previous study has shown to suspend the action of yeast, and of all organised ferments. They also find that when nitrification has thus been suspended for many weeks, it can be restarted by the addition of a small quantity of a nitrifying body. In a second communication they further prove that the temperature of boiling water is sufficient to destroy all power of nitrification, and that soil which has been once heated to this point produces, in air free from germs, carbonic acid and ammonia, but no nitrates. If, however, this soil is moistened with water containing a little unheated soil, the production of nitric acid again commences.

This new theory of nitrification has been investigated at Rothamsted with results completely confirmatory of the views put forward by these French chemists. It was found that the vapour of bisulphide of carbon, and of chloroform, effectually prevented nitrification in a moist garden soil through which air was frequently aspirated, while without these vapours the soil produced nitrates in considerable quantity. A solution of chloride of ammonium containing a little tartaric acid, phosphate of potassium, and carbonate of calcium, was also completely nitrified in a few weeks by the addition of a small quantity of soil taken from the "fairy-ring" of a meadow. This solution, when nitrified, was successfully used as seed to produce nitrification in other similar solutions, which, without this addition, produced no nitric acid. It was further shown that light was prejudicial to nitrification; solutions kept in a dark cupboard producing nitric acid, while similar solutions standing in daylight produced none.

The evidence has thus become very strong that the nitrates in soil owe their origin to oxidation brought about by living organisms. That mycoderms, in their processes of life, may exert a powerful oxidising action upon organc matter, we have already learnt through the researches of Pasteur and others. The most familiar example is that of the acetic fermentation. Vinegar is produced by the oxidation of alcohol during the growth of a very simple organism, the *Mycoderma aceti*, without the growth of such an organism no vinegar is ever formed. It is by similar low organisms that fermentation of all kinds is brought about. Putrefaction has also been shown to be equally dependent on the presence of microscopic organisms, and except under the conditions suitable for their rapid development putrefaction will not take place. With this abundant evidence before us of the energetic decomposition of organic matter, brought about by what we may term microscopic fungi, we can hardly be astonished to find that the same agency is capable of

oxidising the nitrogen of organic matter and of ammonia, and thus producing nitric acid.

The organisms which produce these wonderful changes consist of colourless cells; they are independent of daylight, for they derive their supply of carbon exclusively from organised matter, and from the decomposition of such matter they obtain the force necessary for life and growth. In these respects they differ entirely from green vegetation, in which sunlight is the source of all energy, and carbonic acid gas, decomposed by the aid of light, the material from which carbon is derived. The colourless and green organisms, however, equally require phosphoric acid, potash, and other ash constituents; and both appear to be capable of assimilating nitrogen in the form of ammonia.

Not only are these simple organisms independent of the aid of light, but light is, in some cases at least, actually fatal to their existence. This fact has quite recently been established by Downes and Blunt. They find that the *bacteria* present in an organic fluid may in many cases be entirely destroyed by exposure of the solution to daylight, and that even when this is not the case, their development is much retarded by such treatment. This observation is perfectly in accordance with the fact observed at Rothamsted, that nitrification did not proceed in solutions exposed to daylight. In the last communication of Schloesing and Müntz, it is stated that vegetable soil suspended in water by passing a stream of air through the mixture, undergoes nitrification both in light and darkness. No details of the experiment are given, but it seems probable that such a mixture would be more or less opaque, and the greater bulk of the material consequently at all times in partial darkness.

The microscopic organism producing nitrification has probably distinctive characters, and might be isolated by cultivation under conditions specially suitable to its growth, but more or less unfavourable to the life of other associated germs. Pasteur has pursued this method with success in the case of beer yeast, and has shown that with the pure yeast thus obtained an unchangeable beer may be manufactured, the organisms producing secondary changes having been excluded. The subject of nitrification has clearly reached a stage which demands the aid of the vegetable physiologist. R. WARINGTON

FOSSIL HUNTING AT BOURNEMOUTH

I HAVE recently deposited in the South Kensington Museum some unusually large specimens of fossil plant remains from Bournemouth and Studland. The matrix in which these are imbedded is friable, and the remains, in most cases, are extremely difficult to extract, so that a brief account of the process employed may be of use to would-be collectors. The largest specimen, part of the frond of a feather-palm, measures 4 ft. by 3 ft., and as this presented the greatest difficulties, I will more particularly describe the work which its preservation involved.

In digging last autumn at Bournemouth in a bed of dark clay about 60 feet above the sea-level, and about the same distance from the top of the cliff, we came across a well-preserved fragment of this specimen consisting of a portion of the stem with the bases of pinnæ attached. We included a younger athletic brother, a coast-guardsman whom I have long employed, as well as myself, and occasional other assistance. The tools we used were pick-axes, crow-bar, and spades. The place was a slightly projecting ledge, none too solid, with a steep cliff above and below. So soon as the fragment mentioned was brought to light by a stroke of the pick digging was stopped, and a careful examination was made by the aid of our knives to see in which direction the frond trended. Finding, fortunately, that the direction was towards the mass of the cliff, we determined to use our endeavours to extract it in as perfect a condition as might be. We therefore, at about mid-day, commenced to dig away the superincumbent mass until a slab was bared at least twice the size of that ultimately required, when we proceeded to clear down and lay bare the specimen. Loose sand blowing up in clouds, however, settled upon it and threatened to adhere so firmly to the wet clay that it was feared it might be found impossible to remove it, whilst the drying action of the wind caused it to crack and peel, notwithstanding all our efforts to keep it covered with damp paper and linen. It was then determined to remove the slab without exposing the leaf, leaving that operation until it was safely housed at home, and we therefore commenced the laborious operation of undermining this great slab and removing it in such pieces as from time to time broke away by their own weight from the main mass. For five hours these pieces kept breaking away in blocks of about one foot in thickness, and as much in weight as two or three of us could lift. At dusk our task was not more than two-thirds completed, but as wet was expected, it was determined to extract the whole that night if possible. Perhaps the most toilsome part of the work was carrying the pieces up the sixty feet of cliff. A hand-barrow having been improvised, it required our united efforts to convey each piece to the path above, and this was really hard work, and in addition I had great anxiety throughout lest the edges should be rubbed. Notwithstanding all our trouble we had the mortification of seeing our large lumps repeatedly break and subdivide. The work went on until about 9 P.M., when we found it impossible to continue, and therefore carefully covered up the remains of the slab, the vicinity of a populous town rendering this precaution necessary. The next day the whole of the pieces were removed in a cart from the coast-guards station to an out-house in our occupation. When they arrived there the prospect was far from hopeful. We had apparently but a truckful of lumps of black wet clay, a foot or more in thickness, and varying in diameter from a few inches to two or three feet, the majority without trace of the fossil upon them, or any marks or indication of how they were to be fitted together. Experience among these fossils has taught me not easily to despair, and I knew, moreover, from the care that had been taken, that the edges could not be much abraded, nor could any considerable pieces be missing. Our lodging contained a new and comparatively well-lit cellar, to which all was removed. A table was next made, six feet long and four feet wide, and portions of three days occupied in ascertaining how the pieces could be fitted together.

Two days were then lost in fastening the smaller pieces together into larger slabs, but it was found that these larger pieces would not come together properly in the box, their relative thickness, &c., being different. They were next reduced in thickness to about three inches and transferred to the box in which they now are, and fitted together as accurately as possible and fixed by glue and plaster of Paris, $\frac{3}{4}$ cwt. of the latter being used.

A great disappointment now awaited us. From standing and kneeling upon the slab whilst engaged in digging it out, the upper surface of the leaf was kneaded into the under surface, and would not part for weeks afterwards, until quite dry, and then in very small fragments only.

Another difficulty was that two other fronds were found at lower levels traversing the one we were endeavouring to save, and in some places these had been cleaned out before the mistake was discovered. The base of the frond, it will be seen, has been abandoned altogether, and not more than two-thirds is now preserved. The next thing was to get it to London safely, and the railway officials were cautioned as to the care required and the necessity of keeping it flat and right side up, and the case was